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Interactive comment on "West Greenland ice sheet retreat history reveals elevated precipitation during the Holocene thermal maximum" *by* Jacob Downs et al.

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SUMMARY

This paper focuses on using a combination of paleo observations, ice sheet modeling, and inverse and uncertainty quantification methods to quantify changes in precipitation accompanying the Holocene thermal maximum (HTM) in West-central Greenland. Specifically, a (possibly zero) precipitation anomaly, not accounted for in standard scalings between temperature and precipitation, is proposed under the hypothesis that positive precipitation anomalies are required to explain the reduced rates of margin retreat (due to relatively increased precipitation) indicated by paleo observations around

this time period. The broader context of the study is that such a mechanism could provide a (slight) negative feedback on Greenland margin retreat under current and future warming.

An inverse modeling method that uses the "unscented transform" is devised and applied in order to determine the unknown precipitation anomaly history. Significantly, the proposed approach allows for confidence estimates by providing a distribution of possible histories (as opposed to a single best match). To my knowledge, the approaches devised here and the application are entirely novel within the ice sheet modeling community. The proposed methods are successful at inverting for a physically reasonable precipitation anomaly history, which indeed shows that an increase is required during the HTM. The results are robust with respect to model- and observation-based uncertainty, and choice of methodology. Yet, perhaps unsurprisingly, the results are also fairly sensitive to the choice of temperature history that is assumed (not treated as a free variable in this study).

GENERAL COMMENTS

Comments are referenced to a particular page and line number using, e.g. "5, 3-7:", referring to page 5, lines 3 through 7.

Introduction

2,7: It would be useful to expand on the end of the sentence "…an increase in Arctic precipitation", and include some of the mechanisms by which that might occur (e.g, atmos able to carry more water vapor, more water vapor available due to uncovered and warmer oceans, etc). This could help the reader to have a better understanding for what processes you are trying to account for with your general precip-temperature scaling vs. what you are assuming might fall into the precip. anomaly value.

2,9-10: "… a means of assessing the …". Maybe better to say, a means of assessing how the ice sheet will respond to warmer (and possibly) wetter future climate. The issue of overall ice sheet “stability” seems a bit further removed from this particular topic.

2, 11-12: Do you mean specifically during the HTM? It would be helpful to attach a time span to this statement.

2,14: “. . . due to the presences of more surface water . . .”. This seems to imply a connection to ice sheet dynamics (via lubrication and sliding). Or, do you simply mean increased melting as a proxy for increased mass loss?

2,23-26 (last sentence: You might capture the attention of a few more interested readers here if you include some estimate for the order of magnitude reduction in the no. of fwd model evaluations required (e.g., is 10x less? 100x less?).

General comment on the increased precipitation / decreased rate of margin migration hypothesis âĂŤ It is not discussed whether or not the increased precipitation is assumed to fall as rain or snow. Presumably both could occur, in which case it seems like the precipitation anomaly could have either negative or positive feedbacks w.r.t. the base-rate of margin retreat. That is, precipitation as snow would favor increased SMB, a brake on margin retreat from melting. But, increased precipitation as rain (which is possible at lower elevations) could actually lead to increased marginal melting (or due to changes in ice dynamics). This may not apply during the HTM, e.g. if temperatures are always <= freezing all the way down to sea level. But this could be something to consider in terms of the analogy for how the mechanism might operature under future warming.

Ice sheet model

3,2-3: Minor word choice detail, but maybe better to note as "Tau_b represents the basal shear stress, Betaˆ2 represents the ...", etc. Also, formally, I believe that basal "traction" has a definition - it’s the basal stress tensor (not just the basal shear stress, unless you are only using that component of the tensor) dotted with the normal vector

at the bed, which gives the basal "traction" components (that is, components of the traction vector). Here Bˆ2 would be more appropriately listed as the "basal traction parameter”.

3,5-6: This seems like a complicated way of just saying that effective pressure is a function of (scaled to) the ice thickness along flow.

3,6: “Basal traction” âĂŤ as noted above. The traction parameter what is being tuned here.

3,5-6: “roughly match” - This could benefit from something a bit more quantitative (e.g. provide an RMS or something similar).

PDD model

3,11-12: This is a bit unclear. You are creating an annual climatology composed of monthly averages?

3,19: “We take lambda_p . . .”. Is there a basis for this choice (e.g., a references) or is it arbitrary?

Modeling Approach

5,15-16: Why 5 km beyond the 11.6 ka margin position rather than as close as possible to the inferred margin position?

5,16: replace “ . . . using a fixed ..” with "using methods discussed in Section 2.5”. This could address the redundant text comment below.

5,17: Clarify: Do you mean “a steady rate of retreat in order to give 5 km of total retreat over 1000 yrs.” (?). I think this is just worded oddly (e.g., 5 km is not a retreat rate).

5,17-18: “. . . using the methods . . .”. This is redundant with material in the prev. paragraph.

5,18-19: “This initialization procedure is intended . . .”. This is a little confusing as we don't really know what the initialization procedure is yet (it's discussed in the next section).

Data Assimilation Approach

6,4: Most readers will be curious about the name, "unscented transform". Might be worth a footnote?

6, eqn. 6: X_0 is a scalar (the initial value of the vector x_i)? Or, is it a vector (because it's bold)? Further below, it looks like you define this as a mean.

Section 2.5.1: It would be helpful if you could supply a figure, e.g. in an appendix, with a simple demonstration of what the unscented transform does for some simple (e.g. 2d) function.

9,17: "We randomly sample curves . . .". Be explicit about what exactly "curves" is referring to here.

9,23-24: "Using a low temporal resolution . . .". Is the number of forward model evaluations required equal to the number of points in the delta_P vector?

10,9-10: "The data assimilation procedure can be modified to account for . . .". Precede this sentence with one explaining your interest or need for doing this. I.e., make it clear that you also want to try to account for model uncertainty, not just observational uncertainty.

10, eqn. 23: Is the vector missing a comma to separate items within it? The same question applies to some vector terms included in lines 18 on this same page.

Results

11, 14: As currently written, it's confusing that in this section the Buizert temperature history is introduced and discussed first, and then only after is the Dahl-Jensen temperature history mentioned. It reads a bit as if there was an initial choice made to use Buizert, but then when the results weren't favorable, you tried something else. This im-

pression could be avoided by simply introducing the temperature history as yet another source of uncertainty that you are investigating here (albeit in a non-statistically robust way, since there's only a sample of 2). Note that consistently addressing this problem might require some minor changes to all of the previous sections. That is, while the Buizert history is mentioned multiple times before the results section, the first time we see / hear anything about the Dahl-Jensen history is in the results section.

11,12-13: There should be some discussion regarding why the Holocene precip. anomaly using the Buizert temperature reconstruction is in general, high prior to 10 ka BP (and generally falling thereafter up until ∼8.5 ka BP).

11,24-25: Here, presumably you mean explicitly that you see this relationship IF using the Dahl-Jensen temperature history? Again, this feels a bit contrived as all of the material regarding the temperature history up to this point is w.r.t. the Buizert temperature reconstruction alone.

Discussion

12,25-26: "We find that . . ." I think this requires additional clarification. That is, specify that precipitation during the HTM had to be higher (positive anomalies) relative to the precipitation values one would estimate based on standard air temperature vs. precipitation scalings.

12, 31: "Due to high snowfall in the early Holocene . . .". It would help to add some additional discussion for why exactly this is the case, and why it also doesn't occur when using the Dahl-Jensen temperature reconstruction. This is confusing as the delta_T values prior to ∼9 ka BP appear similar in the two temperature records.

13,3-4: Another way to state this (or, a conclusion that is also relevant to state) is that mulitiple temperature and preciptation histories are consistent with the inferred history of margin position. This is not necessarily a surprising outcome from using an inverse method.

Conclusions

13,29-30: Can you say anything about if / how these methods would work for higher dimensional parameter spaces? It's not entirely clear to me how many params. were optimized with uncertainty here.

14,1: "for some problems" âĂŤ Elaborate on which types of problems. You mean it has advantages for small param. space problems?

14,9-11: Again, it would be nice to be explicit (if you can) about where you think the dividing line is between problem (parameter) sizes that are or are not tractable with this method.

14,26-29: An alternate or additional point her might be that if you were to include temperature as an additional random variably, you might be able to use it to better determine which, if either, of the temperature histories is consistent with the margin retreat history.

General comments for conclusions section:

The two temperature records here are inferred in different ways and also apply to different regions (one at the ice divide, and one for all of Greenland). Do you expect that a regionally-focused temperature history (e.g. for West central Greenland) might be needed to increase confidence in the results (since they are quite different)? Do you expect the the two records here maybe at least bracket the possible values of delta_P?

The central question posed at the beginning of the paper is left hanging a bit. Does this work argue for a strong negative feedback operating to slow the retreat of (terrestrial margins of) the Greenland ice sheet as Arctic climate continues to warm? If so, then you should say so. Right now, your results sort of leave the door open to interpreting this work either way, which could be dangerous (in terms of letting someone else interpret your results however they see fit). If you think the results are unequivocal in that sense, then it might be good to be explicit about that too.

Minor / Editorial-level Comments

Minor or editorial-level comments are included as edits directly to the pdf file (attached).

Figures

Figure 2: Are blue regions intended to identify areas below sea level and in contact with the ocean or just the former? The way this is labeled, "bedrock elevation and sea level changes" , is a bit confusing. I can see someone interpreting the blue areas behind the terminus in the 0 ka BP plot as being connected to the ocean, when I think you are really just indicating that they are below sea level. This makes it a bit unclear if you are changing sea level in your simulations or not (i.e., if that is part of the forcing or if you are always treating this as a terrestrial margin).

Figure 4: This figure caption would benefit from some additional clarification as to what is being shown. These are just 4 samples of glacier length history for four possibly different delta_P histories that make up the larger distribution, correct? It took me a while to figure this out. You could also be explicit about which set of lines goes with which vert axis (left or right). Also, you might consider using a diff. set of colors (or simply diff. line types?) as it is tempting to try to connect these colored lines with the margin positions shown in earlier Figs (the no. of samples shown here and the colors are similar to the no. of margin positions and the colors).

Figure 5: As in the previous figure, make it clear that the colored lines here are just a few representative histories that are part of the part of the broader distribution.

Figure 6: While it might make the plots look a bit odd, I think it would be better if panels c and d had the same vertical and horiz. axis limits, in order to make comparison between the two easier. Same goes for panels a and b. If nothing else, you could try to include a box in panel c that shows where the corresponding area of panel d is.

Figure 9: ". . . which uses a fixed parameter set . . ." Is this equivalent to assuming that the fwd model has no uncertainties associated with it (i.e., the model is perfect and

only the obs have errors)?

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2019-129/tc-2019-129-RC1-supplement.pdf

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-129, 2019.